MR. BALFOUR ON SCIENTIFIC PROGRESS.[1]

APART altogether from individual likes and dislikes, is there any characteristic note which distinguishes this century from any that have gone before it?

On this point I range myself with those who find the characteristic note in the growth of science. In the last 100 years the world has seen great wars, great national and social upheavals, great religious movements, great economic changes. Literature and art have had their triumphs, and have permanently enriched the intellectual inheritance of our race. Yet, large as is the space which subjects like these legitimately fill in our thoughts, much as they will occupy the future historian, it is not among these that I seek for the most important and the most fundamental differences which separate the present from preceding ages. Rather is this to be found in the cumulative products of scientific research, to which no other period offers a precedent or a parallel. No single discovery, it may be, can be compared in its results to that of Copernicus; no single discoverer can be compared in genius to Newton; but, in their total effects, the advances made by the nineteenth century are not to be matched. Not only is the surprising increase of knowledge new, but the use to which it has been put is new also. The growth of industrial invention is not a fact we are permitted to forget. We do, however, sometimes forget how much of it is due to a close connection between theoretical knowledge and its utilitarian application which, in its degree, is altogether unexampled in the history of mankind. I suppose that, at this moment, if we were allowed a vision of the embryonic forces which are predestined most potently to affect the future of mankind, we should have to look for them, not in the Legislature, nor in the Press, nor on the platform, nor in the schemes of practical statesmen, nor the dreams of political theorists, but in the laboratories of scientific students whose names are but little in the mouths of men, who cannot themselves forecast the results of their own labours, and whose theories could scarce be understood by those whom they will chiefly benefit.

I do not propose to attempt any sketch of our gains from this most fruitful union between science and invention. I may, however, permit myself one parenthetic remark on an aspect of it which is likely more and more to thrust itself unpleasantly upon our attention. Marvellous as is the variety and ingenuity of modern industrial methods, they almost all depend in the last resort upon our supply of useful power; and our supply of useful power is principally provided for us by methods which, so far as I can see, have altered not at all in principle, and strangely little in detail, since the days of Watt. Coal, as we all know, is the chief reservoir of energy from which the world at present draws, and from which we in this country must always draw; but our main contrivance for utilising it is the steam engine, and, by its essential nature, the steam engine is extravagantly wasteful. So that, when we are told, as if it was something to be proud of, that this is the age of steam, we may admit the fact, but can hardly share the satisfaction. Our coalfields, as we know too well, are limited. We certainly cannot increase them. The boldest legislator would hesitate to limit their employment for purposes of domestic industry. So the only possible alternative is to economise our method of consuming them. And for this there would, indeed, seem to be a sufficiency of room. Let a second Watt arise. Let him bring into general use some mode of extracting energy from fuel which shall only waste eighty per cent. of it, and lo! your coalfields, as sources of power, are doubled at once. The hope seems a modest one, but it is not yet fulfilled; and therefore it is that we must qualify the satisfaction with which at the end of the century we contemplate the unbroken course of its industrial triumphs. We have, in truth, been little better than brilliant spendthrifts. Every new invention seems to throw a new strain upon the vast, but not illimitable, resources of nature. Lord Kelvin is disquieted about our supply of oxygen; Sir William Crookes about our supply of nitrates. The problem of our coal supply is always with us. Sooner or later the stored-up resources of the world will be exhausted. Humanity, having used or squandered its capital, will thenceforward have to depend upon such current income as can be derived from that diurnal heat of the sun and the rotation of the earth till, in the sequence of the ages, these also begin to fail. With such remote speculations we are not now concerned. It is enough for us to take note how rapidly the prodigious progress of recent discovery has increased the drain upon the natural wealth of old manufacturing countries, and especially of Great Britain, and, at the same time, frankly to recognise that it is only by new inventions that the collateral evils of old inventions can be mitigated; that to go back is impossible; that our only hope lies in a further advance.

After all, however, it is not necessarily the material and obvious results of scientific discoveries which are of the deepest interest. They have effected changes more subtle and perhaps less obvious which are at least as worthy of our consideration and are at least as unique in the history of the civilised world. No century has seen so great a change in our intellectual apprehension of the world in which we live. Our whole point of view has changed. The mental framework in which we arrange the separate facts in the world of men and things is quite a new framework. The spectacle of the universe presents itself now in a wholly changed perspective. We not only see more, but we see differently. The discoveries in physics and in chemistry, which have borne their share in thus re-creating for us the evolution of the past, are in process of giving us quite new ideas as to the inner nature of that material whole of which the world's traversing space is but an insignificant part. Differences of quality once thought ultimate are constantly being resolved into differences of motion or configuration. What were once regarded as things are now known to be movement. Phenomena apparently so wide apart as light, radiant heat and electricity, are, as it is unnecessary to remind you, now recognised as substantially identical. From the arrangement of atoms in the molecule, not less than their intrinsic nature, flow the characteristic attributes of the compound. The atom itself has been pulverised, and speculation is forced to admit as a possibility that even the chemical elements themselves may be no more than varieties of a single substance. Plausible attempts have been made to reduce the physical universe, with its infinite variety, its glory of colour and of form, its significance and its sublimity, to one homogeneous medium in which there are no distinctions to be discovered but distinction of movement or of stress. And although no such hypothesis can, I suppose, be yet accepted, the gropings of physicists after this, or some other not less audacious unification, must finally, I think, be crowned with success. The change of view which I have endeavoured to indicate is purely scientific, but its consequences cannot be confined to science. How will they manifest themselves in other regions of human activity, in literature, in art, religion? The subject is one rather for the lecturer on the twentieth century than for the lecturer on the nineteenth. I, at least, cannot endeavour to grapple with it.

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[1] Address delivered by Mr. Balfour, M.P., at the opening of the Cambridge Summer Meeting on August 2. Abridged from the *Times*.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, June 14.—"The Electrical Effects of Light upon Green Leaves." By Augustus D. Waller, M.D., F.R.S.

In the preliminary communication recently made to the Royal Society, the author shows how, from the study of the electrical effects of light upon the retina, he was led to ask whether the chemical changes aroused by the action of light upon green leaves are also accompanied by electrical effects demonstrable in the same way as the eye currents. The question is tested in the following way:—A young leaf freshly gathered is laid upon a glass plate and connected with a galvanometer by means of two unpolarisable clay electrodes A and B. The half of the leaf connected with A is shaded by a piece of black paper. An inverted glass jar forms a moist chamber to leaf and electrodes, which are then enclosed in a box provided with a shuttered aperture through which light can be directed. A water trough in the path of the light serves to cut out heat more or less. Under favourable conditions there is obtained with such an arrangement a true electrical response to light, consisting in the establishment of a potential difference between illuminated and non-illuminated half of a leaf, amounting to 0·02 volt.

The deflection of the galvanometer spot during illumination is such as to indicate current in the leaf from excited to protected part. The deflection begins and ends sharply with the beginning and end of illumination; it is provoked slightly by diffuse daylight, more by an electric arc-light, most by bright sunlight. It is abolished by boiling the leaf, and by the action of an anæsthetic, carbon dioxide.

The first experiments, made at the end of March, were upon iris leaves taken from plants about 6 inches high, and the response to light was then between 0·001 and 0·002 volt in value. Experiments upon similar leaves were resumed early in May, when it appeared that the external condition by which the state of the leaf is most obviously governed is *temperature*. On warm days the response ranged from 0·005 to 0·02 volt; on cold days it did not rise above above 0·005, and was sometimes nil. Some tests upon leaves in a warmed box gave satisfactory results, which may be thus summed up:—The normal response at 15°–20° C. is diminished or abolished at low temperature (10°), augmented at high temperature (30°), diminished at higher temperature (50°), and abolished by boiling.

As the month of May advanced, the iris leaves, even in the warm box, became more and more inert, and by the 23rd inst., when the plants were mostly full grown and in flower, no satisfactory leaf could be found. Leaves of iris appear to give more marked response at or about mid-day, than at or about 6 p.m. Tested by Sach's method the leaves gave no evidence of starch activity during insolation.

On the failure of the iris leaves to react, other leaves were sought for which should give evident differences of reaction in correlation with evident differences of state. Leaves of tropæolum and of mathiola gave a response to light contrary in the main to the ordinary iris response, viz. "positive" during illumination, and subsequently "negative."[1] In these two cases leaves empty of starch acted better than leaves laden with starch. Leaves of begonia gave a variety of responses strongly suggestive of the simultaneous action of two opposed forces effecting a resultant deflection in a + or − direction. Leaves of ordinary garden shrubs and trees, &c., *e.g.* lilac, pear, almond, mulberry, vine, ivy, gave no distinct response; this is possibly due to a lower average metabolism in such leaves as compared with the activity of leaves of small young plants in which leaf-functions are presumably concentrated within a smaller area. The petals of flowers gave no distinct response, which indicates that chloroplasts are essential to the reaction.

The effect of carbon dioxide upon the iris leaf was abolition of response during and after passage of the gas, with subsequent augmentation. Upon mathiola and tropæolum, augmentation of response followed on applying air containing 1 to 3 per 100 of carbon dioxide, and prompt abolition resulted from a full stream run through the leaf-chamber. On the air supply being kept clear of carbon dioxide there was gradual abolition of response, followed by gradual recovery on the re-admission of a small amount of carbon dioxide.

"Fatigue" effects may be produced if the successive illuminaations (of 5 minutes duration) are repeated at short intervals (10 minutes). At intervals of 1 hour, successive illuminations of 5 minutes produce approximately equal effects. With the leaf of mathiola periods of illumination of 2 minutes at intervals of 15 minutes were used without provoking any obvious sign of fatigue.

June 21.—"Note on Inquiries as to the Escape of Gases from Atmospheres." By G. Johnstone Stoney, M.A., Hon. D.Sc., F.R.S.

Three investigations have been published which profess to supply information about the escape of gases from atmospheres. Two of them, those of Messrs. Cook[2] and Bryan,[3] while differing in other respects, agree in reasoning forwards by the help of the kinetic theory of gas from the supposed causes; the third[4] pursues a method regarded as trustworthy by the present writer, and reasons backwards by the help of the same theory from the observed effects.

Where, as in the present instance, the *a priori* and *a posteriori* methods have led to inconsistent numerical results, it is incumbent upon us to search for the mistake or mistakes which must somewhere have been made. If these can be found and corrected, an important advantage is gained; and the present is an attempt to trace some of them by inquiring whether there are conditions or agencies in nature which facilitate the escape of gaseous molecules from the earth, and which are omitted, or which have not been sufficiently taken into account, in Mr. Cook's and Prof. Bryan's investigations.

Let ΔV be a volume containing at a given epoch a large number n of molecules of the atmosphere, and let Δt be a duration commencing at that instant. Also, let n' be the number of encounters which each of these molecules on the average meets with in the times Δt. Then will

$$N = nn'$$

be the total number of their free paths in that time; and the actual number of these free paths, in which the initial speed after an encounter lies at the time t between v and $v+dv$, must be precisely

$$dN = N(\pi+\delta)dv, \qquad (1)$$

where π is the probability function (that employed by Mr. Cook, or that employed by Prof. Bryan, or some other), and δ (the deviation function) represents whatever is the real divergence of the actual number from that computed by the formula used by them, viz.:

$$dN = N\pi dv; \qquad (2)$$

in other words, computed on the supposition that δ/π is of negligible amount.

Now π is one fully-determined function in Mr. Cook's investigation, and another fully-determined function in Prof. Bryan's; but little is known of what δ is in either case, except that it is in both an excessively complex function of N, v, t, with several other variables, some of which it is difficult even to indicate; and that by its amount for any given value of t and at any given position in the atmosphere it must supply in equation (1) the actual effect, at that time and place, of all natural agencies which had not been taken into account in calculating the expression π.

If due care has been taken in framing the probability law π, it will in many cases be legitimate to assume that δ/π is sufficiently small to warrant our using equation (2) when computing the approximate distribution among the free paths of those speeds which assign *large* values to π, while at the same time it may need proof and may not be a legitimate assumption in reference to those values of v which make π *small*. Now it is in this latter case that the assumption has to be made by Mr. Cook and Prof. Bryan.

The conditions under which the assumption is likely not to be true are the following:—

A. Where the events, the law of whose distribution purports to be represented by the π function, are of such a kind that a vast number of the events need to be passed under review in order to secure an approximate conformity to *any* fixed law. Now experiment shows that in ordinary air trillions of the free paths, probably many trillions, must be grouped together in order to make manifest any law in the distribution of the speeds. In all such cases we are not entitled to ignore the δ function, except in estimating the frequency of such speeds as can be shown to assign a sufficient preponderance to the π function. Accordingly it is not legitimate to ignore the δ function when treating of the frequency of speeds which make π excessively small, such as are the speeds which carry molecules away from the earth.

B. But a more important omission occurs where the function π has been arrived at without taking into account agencies in nature which affect the distribution of speeds. Where this has been done the δ function must include the whole effect of these agencies, and this again forbids our relying upon equation (2) in computing the frequency of any speed which makes the value of π small.

B 1. Thus in Mr. Cook's computation no notice is taken of the anisotropic character of the outer strata of the earth's atmosphere, which facilitates the escape of molecules. In Prof. Bryan's this is partly taken into account by treating the molecules as moving in a constant field of force. This may possibly be sufficient, though it ignores the reactions which are also necessarily present. To include them it would be necessary to extend the partition of energy beyond the molecules of the atmosphere to all the other molecules of the earth which attract them.

B 2. Then, again, both computations ignore the incessant turbulence of the atmosphere which, in its lower strata, produces all the phenomena of weather, and in its upper regions phenomena which are swifter and on a larger scale. This turmoil, with all its dynamical, thermal and electrical effects, is

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[1] "Negative" as the term is employed in physiological literature, *i.e.* negative pole of positive element ("zincative").

[2] *Astrophysical Journal* for January 1900.

[3] *Roy. Soc. Proc.*, April 5, 1900, p. 335.

[4] *Scientific Transactions of the Royal Dublin Society*, vol. vi. Part 13; or *Astrophysical Journal* for January 1898. And for further evidence that helium is escaping from the earth, see NATURE of May 24, 1900, p. 78.

due, like most other events upon the earth, to the shiftings about of energy which intervene between the advent of energy and from the sun and its radiation from the earth into space; and to take it into account in an investigation based on the laws of the partition of energy, it would be necessary to extend that partition beyond the earth to the sun and to the intervening æther.

B 3. So, again, the great absorption of solar radiation which takes place in the outer layers of the earth's atmosphere will have to be taken into account, and as it has not been included under function π, it still further augments the part which the δ function takes in equation (1) and renders equation (2) an insufficient one for the purposes of the investigation.

B 4. The commotion going on in the atmosphere consists in part of electrical phenomena. Some of these—thunderstorms, auroras, the electrical condition of fogs, &c.—can be observed from the stations which men occupy at the bottom of the atmosphere, and are of such a kind that they must be accompanied by a charged condition of that stratum of the atmosphere the density of which renders it a better conductor than the atmosphere above it and below. This stratum, then, and the strata above it receive charges of electricity which, according to the varying condition of the strata further down, will sometimes be disguised electricity and at other times undisguised. This electrified condition of the upper regions, co-operating with ascending currents, which necessarily increase in speed as they advance, will presumably give rise to prominences upon the earth's atmosphere, upon which the density of the electrification will be intensified and from which in consequence gaseous molecules find it easier to escape than from other situations. In this and other ways electricity may help the escape.

Now of these agencies, all of which affect the rate at which gas can escape from the earth, none is included in the investigation which Mr. Cook has made of that phenomenon; and only the first (B 1) is dealt with by Prof. Bryan. Moreover, it is probable that these are not the only ways in which nature can intervene, and which have been overlooked. The supposition then that either of the probability laws made use of by those investigators can be applied to our actually existing atmosphere, without a large correcting function δ, would appear to be a mistake; and, if so, the inferences from those laws when so applied are not part of a real interpretation of nature. It need not therefore occasion any surprise that, in the case of helium, the facts of nature seem to negative those inferences. (See NATURE of May 24, 1900; the second column of p. 78.)

EDINBURGH.

Royal Society, July 16.—Lord Kelvin, President, in the chair.—Lord Kelvin read a paper on the motion in an infinite elastic solid by the motion, through the space occupied by it, of a body acting on it only by attraction and repulsion. The ideal atom considered in this paper was a region of space in which the ether was changed in density by the action of forces upon it. In the particular case chosen for development the atom was taken as spherical with spherical distributions of density within it, and every element of matter was supposed to act on every element of the ether according to the Newtonian law. The further assumption was made that the average density of the ether within the atom was the same as if the atom were not present. The atom and the ether were then supposed to be in relative motion, and the total kinetic energy of the ether within the atom was calculated, as also the effective inertia of the ether in the space occupied by the matter. On the assumption that the density of the ether at the centre of the atom was 101 times greater than the undisturbed density, it was found that a refractivity was obtained a little smaller than that of oxygen. By assuming that the average density was in excess or defect of the undisturbed density of the ether, we could extend the method so as to include electrical actions.—In a second paper, on the number of molecules in a cubic centimetre of gas, Lord Kelvin pointed out that in the preceding paper he had been obliged to take the number as 4×10^{20} instead of Maxwell's number, 19×10^{18}.—In a paper on the hyperbolic quaternion, Dr. Alex. Macfarlane showed how by the introduction of "real" instead of "imaginary" vectors, quaternion theorems of spherical geometry could be generalised so as to be applicable to hyperbolic geometry.—Sir John Murray and Dr. Philippi communicated a preliminary note on the deep-sea deposits collected during the *Valdivia* expedition of 1898–9. Leaving Hamburg and passing round by the north of Scotland, the *Valdivia* proceeded southwards by the west coast of Africa to the Cape, thence to the Antarctic seas, returning by way of the Indian Ocean and the Suez Canal. Generally speaking, the nature of the deposits agreed with what was already known, but fuller information was gained in many instances. For example, off the mouth of the Congo samples of coprolitic mud had been obtained, largely made up of little oval pellets of mud which had passed through the intestines of echinoderms. These had consolidated and were apparently in the process of being transformed into glauconitic and phosphatic concretions. The study of the formation and distribution of glauconite was geologically of great importance, and a detailed examination of the *Valdivia* collections would probably throw much light on the subject.—Prof. J. C. Beattie communicated a second part of his researches into the leakage of electricity from charged bodies at moderate temperatures. In most of the experiments described, zinc strips resting on insulated iron plates were sprinkled with various salts and then heated to about 350° C., the whole being enclosed in an iron box which was connected to the case of the electrometer. Among the substances used were common salt, alone or with iodine or bromine, and similar combinations with the chlorides of lithium, lead, potassium, &c. Generally a steady negative charge was produced by the heating, but not always. The difference of potential so obtained depended on the nature of the insulated metals, but not on their distance apart. When high voltages were used, the positive charge leaked away, while the negative charge was retained. An explanation was offered founded on Enright's and on Townsend's experiments.—A communication was also presented by Dr. Thomas Muir on the theory of skew determinants and pfaffians in the historical order of its development up to 1857.—In a brief review of the session, the President referred to the great losses the Society had sustained through the deaths of the Duke of Argyll and Sir Douglas Maclagan.

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